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Chapter 50

What Is Innate and Why: Comments on the Debate

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I can say in a nutshell what I think about Chomsky and Piaget; neither has good arguments, but there is almost certainly something to what each one says. In this paper I am first going to say why the arguments are not good, and then discuss the more important question of why there is something right to what they say.

I shall begin with Chomsky's arguments. When one reads Chomsky, one is struck by a sense of great intellectual power; one knows one is encountering an extraordinary mind. And this is as much a matter of the spell of his powerful personality as it is of his obvious intellectual virtues: originality; scorn for the faddish and the superficial; willingness to revive (and the ability to revive) positions (such as the "doctrine of innate ideas") that had seemed passé; concern with topics, such as the structure of the human mind, that are of central and perennial importance. Yet I want to claim that his individual arguments are *not good*. I will examine only one example here, but I claim that a similar examination could be carried out on any of the arguments he has offered at this conference, with similar results.

The argument concerns "the process of formation of simple yes-or-no questions in English." In his paper, Chomsky considers "such declarative-question pairs" as:

- (1) The man is here.—Is the man here?
The man will leave.—Will the man leave?

And he considers two hypotheses "put forth to account for this infinite class of pairs" (of course, H_1 has never been "put forth" by anyone, nor would any sane person put it forth):

- H_1 : process the declarative from beginning to end (left to right), word by word, until reaching the first occurrence of the words *is*, *will*, etc.; transpose this occurrence to the beginning (left), forming the associated interrogative.
 H_2 : same as H_1 , but select the first occurrence of *is*, *will*, etc., following the first noun phrase of the declarative.

Chomsky then writes:

Let us refer to H_1 as a "structure-independent rule" and H_2 as a "structure-dependent rule." Thus, H_1 requires analysis of the declarative into just a sequence of words, whereas H_2 requires an analysis into successive words and also abstract phrases such as "noun phrase." The phrases are "abstract" in that their boundaries and labeling are not in general physically marked in any way; rather, they are mental constructions.

A scientist observing English speakers, given such data as (1), would naturally select hypothesis H_1 over the far more complex hypothesis H_2 , which postulates abstract mental processing of a nontrivial sort beyond H_1 . Similarly, given such

data as (1) it is reasonable to assume that an "unstructured" child would assume that H_1 is valid. In fact, as we know, it is not, and H_2 is (more nearly) correct. Thus consider the data of (2):

- (2) The man who is here is tall.—Is the man who is here tall?
The man who is tall will leave.—Will the man who is tall leave?

These data are predicted by H_2 and refute H_1 , which would predict rather the interrogatives (3):

- (3) Is the man who here is tall?
Is the man who tall will leave?

Now the question that arises is this: how does a child know that H_2 is correct (nearly), while H_1 is false? It is surely not the case that he first hits on H_1 (as a neutral scientist would) and then is forced to reject it on the basis of data such as (2).

Chomsky's conclusion from all this is the following:

Such observations suggest that it is a property of S_0 —that is, of $LT(H,L)$ —that rules (or rules of some specific category, identifiable on quite general grounds by some genetically determined mechanism) are structure-dependent. The child need not consider H_1 ; it is ruled out by properties of his initial mental state, S_0 .

I wish to discuss this example by considering two different questions: (1) can we account for the child's selection of "structure-dependent" hypotheses and concepts in the course of language learning on the basis of general intelligence, without postulating that the preference for H_2 over H_1 is built in, or that a template of a typical human language is built in, as Chomsky wishes us to do; and (2) can we account specifically for the preference of H_2 over H_1 without assuming that such a specific preference is built in? Before discussing these questions, however, I want to consider the vexed question, "What is a grammar?"

The Nature of Grammars

A grammar is some sort of system which—ideally—generates the "grammatical sentences" of a language and none of the ungrammatical ones. And a grammatical sentence is one generated by *the* grammar of the language (or by any adequate one, if one believes as Zellig Harris does that there is no such thing as *the* grammar of a language).¹ This is obviously a circular definition. But how does one break the circularity?

Chomsky suggested long ago (in "Explanatory Models in Linguistics")² that a child *hears* people classing sentences as "grammatical" or "ungrammatical"—not, of course, in those words, but by hearing them correct each other or the child—and that he projects a grammar as a simplest extrapolation from such data satisfying some innate constraints.

The trouble with this view is that the factual premise is clearly false. People don't object to all and only *ungrammatical* sentences. If they object at all, it is to *deviant* sentences—but they do not, when they correct each other, clearly say (in a way that a child can understand) whether the deviance was syntactic, semantic, discourse-theoretic, or whatever.

Chomsky asserts that the child is, in effect, supplied with "a list of grammatical sentences" and "a list of ungrammatical sentences" and has to extrapolate from these

two lists. But this is surely false. If anything, he is supplied rather with a list of acceptable sentences and a list of sentences that are deviant-for-some-reason-or-other; a grammar of his language will generate (idealizing somewhat) all of the acceptable sentences in the first list, but unfortunately, it will not be the case that it generates none of the deviant sentences in the other list. On the contrary, the grammatical sentences will be a superset of the (finite list of) acceptable sentences, which is *not disjoint* from the (finite list of) deviant sentences.

Moreover, the second list does not have to exist at all. Chomsky has cited evidence that children can learn their first language without being corrected; and I am sure he also believes that they don't need to hear anyone else corrected either. Chomsky might reply to this by scrapping the hypothetical second list (the list of "ungrammatical," or at least, "unacceptable" sentences). He might say that the grammar of an arbitrary language is the simplest projection of any suitable finite set of acceptable sentences satisfying some set of innate constraints. This throws the whole burden of defining what a grammar is on the innate constraints. I want to suggest a different approach: one that says, in quite traditional fashion, that the grammar of a language is a property of the *language*, not a property of the brain of *Homo sapiens*.

Propositional Calculus

Let us start with a simple and well-understood example: the artificial language called "propositional calculus" with its standard interpretation. The grammar of propositional calculus can be stated in many different but equivalent ways. Here is a typical one:

- (I) A propositional variable standing alone is a well-formed formula.
- (II) If A and B are well-formed formulas, so are $\sim A$, $(A \& B)$, $(A \vee B)$ and $(A \supset B)$.³
- (III) Nothing is a well-formed formula unless its being so follows from (I) and (II).

The fact that a perfectly grammatical sentence may be deviant for semantic reasons, which is a feature of natural languages, is possessed also by this simple language, since " $p \& \sim p$ " (for example) is perfectly grammatical but would not be "uttered" for obvious semantic reasons.

Now consider the "semantics" of propositional calculus as represented by the following inductive definition of *truth* in terms of *primitive truth* (truth for propositional variables, which is left undefined). The fact that primitive truth is left undefined means that this can be thought of as an *interpretation-schema*, which becomes an interpretation when joined to any definition of primitive truth.

Definition:

- (i) $\sim A$ is true if and only if A is not true.
- (ii) $(A \& B)$ is true if and only if A and B are both true.
- (iii) $(A \vee B)$ is true if and only if at least one of A, B is true.
- (iv) $(A \supset B)$ is true unless A is true and B is not true.

Notice that the inductive definition of *truth* in propositional calculus parallels (in a sense which could be made precise, but which I will not attempt to make precise here) the inductive definition of *grammatical* in propositional calculus. Now, there are other ways of defining grammatical in propositional calculus with the property that corre-

sponding to them there exist parallel inductive definitions of truth in propositional calculus. But if we limit ourselves to those that are computationally feasible (that is, the corresponding decision program is short, when written in any standard format, and the typical computation is also short), not a great many are known, and they are all extremely similar. In this sense, propositional calculus as an interpreted system possesses an *intrinsic* grammar and semantics.

Let me elaborate on this a little. If Martians exist, very likely they have hit upon propositional calculus, and it may be that when they use propositional calculus their logicians' brains employ different heuristics than our logicians' brains employ. But that does not mean that propositional calculus has a different grammar when used by a Martian and when used by a Terrestrial. The grammar is (any one of) the simplest inductive definition(s) of the set of strings in the alphabet of propositional calculus for which truth is defined—that is, the simplest inductive definition(s) with the property that there exist parallel inductive definitions of truth. Given the semantics of propositional calculus (and no information about the brains of speakers), the class of reasonable grammars is fixed by that semantics, *not* by the structure of the brains that do the processing.

It may seem that I have begged too many questions by introducing the predicate "true"; but it is not essential to my argument. Suppose we do not define "true," but rather "follows from." Any reasonably simple definition of the relation "x follows from y" in propositional calculus will have the property that it presupposes a syntactic analysis of the standard kind. In other words, checking that something is an axiom or a proof, etc., will involve checking that strings and components of strings have the forms $(p \ \& \ q)$, $\sim p$, $(p \vee q)$, $(p \supset q)$. The grammar (I), (II), (III) not only generates the set of strings over which the relation "follows from" is defined, but it generates it in a way that corresponds to properties of strings referred to in the definition of "follows from."

Coming to natural language: suppose we think of a natural language as a very complicated formalized language whose formalization is unknown. (This seems to be how Chomsky thinks of it.) Suppose we think of the speaker as a computer that, among other things, computes whether certain strings are "true," given certain inputs, or if you don't like "true," as a computer that computes whether certain sequences of strings are "proofs," or computes the "degree of confirmation" of certain strings, and so forth. The fact is that any one of these semantic, or deductive logical, or inductive logical notions will have an *inductive definition* whose clauses parallel or at least presuppose a syntactic analysis of the language.

To come right out with it: I am suggesting (1) that the declarative grammar of a language is the inductive definition of a set of strings which is the set over which semantic, deductive-logical, inductive-logical (and so on) predicates are defined;⁴ (2) that it must be in such a form that the inductive definitions of these predicates can easily "parallel" it; (3) that the corresponding decision program must be as computationally feasible as is consistent with (1) and (2). If a language is thought of in this way—as a system of strings with a semantics, with a deductive logic, with an inductive logic, and so on—then it is easy to see how the grammar can be a property of the *language* and not of the speakers' *brains*.

The Nature of Language Learning

Let us consider the linguistic abilities of Washoe (the chimpanzee brought up to use a certain amount of deaf-mute sign language by Alan and Beatrice Gardner). No doubt Chomsky will point out that Washoe lacks many of the syntactic abilities that humans

have, and on these grounds he would claim that it is wrong to apply the term “language” to what she has learned. But the application of this term is not what is important. What is important is the following:

1. There is a certain class of words, which I will call *nouns-for-Washoe*, which Washoe associates with (classes of) *things*. For example, Washoe associates the word “grape” (in sign language) with more-or-less stereotypical grapes, “banana” with more-or-less stereotypical bananas, and so forth.
2. There is a *frame*, ____ gives ____ (to) ____, which Washoe has acquired (for example, “Alan gives apple to Trixie”).
3. She can project *new* uses of this frame. If you teach her a new word, say “date,” she will figure out herself the use she is expected to make of “____ gives *date* (to) ____”
4. She can use the word “and” to combine sentences. She can figure out the expected use of *p* and *q* from the uses of *p* and *q* separately.⁵

Actually Washoe’s abilities go far beyond these four capacities; but let us just consider these for now. The only plausible account of what has occurred is that Washoe has “internalized” a rule to the effect that if *X* is a *noun-for-Washoe*, and *A*, *B*, and *C* are people’s names—counting Washoe (of course) as a person—then “*A* gives *X* to *B*” is a sentence, and a rule to the effect that if *p*, *q* are sentences so is *p and q*. And these are *structure-dependent rules* which Washoe has learned *without benefit of an innate template for language*.

Nor is this really surprising. Let us introduce a semantic predicate to describe the above tiny fragment of Washoe’s “language” (where the “shudder-quotes” are inserted to avoid the accusation of question-begging), say, the predicate “corresponds to the condition that.” Here are the “semantic rules” for the fragment in question:

- (I) If *X* is a *noun-for-Washoe* and *B*, *C* are people-names, and *X* corresponds to things of kind *K*, and *b*, *c* are the people corresponding to *B*, *C*, then “*B* gives *X* (to) *C*” corresponds to the condition that *b* gives something of kind *K* to *c*.
- (II) If *p*, *q* are *sentences-for-Washoe*, *p and q* corresponds to the condition that the condition corresponding to *p* and the condition corresponding to *q* both obtain.

Now, I submit that Washoe is not really interested in learning that certain *uninterpreted* strings of gestures have a certain *uninterpreted* property called “grammaticality.” She is interested for practical reasons—reward, approval, and so forth—in learning (I) and (II). But learning (I) and (II) automatically involves learning the grammatical facts that:

- (i) If *B*, *C* are people-names and *X* is a *noun-for-Washoe*, “*B* gives *X* (to) *C*” is a *sentence-for-Washoe*.
- (ii) If *p*, *q* are *sentences-for-Washoe*, so is *p and q*.

For the set of sentences “generated” by the “grammar” (i), (ii) is precisely the set over which the semantic predicate—“corresponds to the condition that ____”—is defined by the inductive definition (I), (II); and the clauses (I), (II) presuppose precisely the syntactic analysis given by (i), (ii). Given that Washoe is trying to learn the *semantics* of Washoe-ese, and the syntax is only a *means* to this end, there are only two possibilities: either her intelligence will be too low to internalize “structure-dependent” rules like (I), (II), and she will fail; or her intelligence will be high enough, and as a corollary we will be able to ascribe to Washoe “implicit knowledge” of the syntactic rules (i), (ii)—not

because she "knows" (I), (II) *and in addition* "knows" (i), (ii), but because having the "know-how" that constitutes implicit knowledge of (I), (II) *includes* implicit knowledge of (i), (ii).

But the same thing is true of the child. The child is not trying to learn a bunch of *syntactic* rules as a kind of crazy end-in-itself. He is learning, and he wants to learn, *semantic* rules, and these *cannot* be stated without the use of structure-dependent notions. There aren't even plausible candidates for structure-independent semantic rules. So *of course* (given that his intelligence is high enough to learn language), *of course* the child "internalizes" structure-dependent rules. And given that he must be building up an "inner representation" of abstract structural notions such as *sentence, noun, verb phrase*, and so on in learning to understand the language, the mere fact that H_2 uses such notions and H_1 does not, does *not* make H_2 so much less plausible than H_1 .

Chomsky has, so to speak, "pulled a fast one" on us. He presents us with a picture of the child as being like an insanely scientific linguist. Both are looking at language as a stream of uninterpreted noises; both are interested in an occult property of "grammaticality." From this (crazy) point of view, it is not surprising that H_1 seems infinitely "simpler" than H_2 . So—Chomsky springs his carefully prepared trap—"Why doesn't the child try the simpler-but-false hypothesis H_1 *before* the correct hypothesis H_2 ?"

But this isn't what children (or sane linguists) are like at all. The child is in the process of trying to understand English. He has already tumbled (if Washoe can, so can he!) to the fact that he needs to internalize structure-dependent notions to do this. So the mere fact that H_2 *uses* such notions doesn't at all make it implausible or excessively complex. The point is that the *learning of grammar is dependent on the learning of semantics*. And there aren't even any candidates for structure-independent semantic rules (if there are, they get knocked out pretty early, even by a chimpanzee's brain).

H₁ Considered More Closely

So far I have argued that H_2 is not nearly as weird from the point of view of the intelligent brain unaided by an innate template of language as Chomsky wants to make it seem. But I haven't argued against H_1 . So still the question remains, why doesn't the child try H_1 ?

Let us try applying to this problem the conception of grammar we just sketched (grammar as, so to speak, semantics minus the semantic predicates). H_1 will only be "tried" by the child if the child "tries" some *semantic* hypotheses that correspond to H_1 . The child wants to *understand* questions, not just to "flag" them as questions. But it is plausible to assume (and Chomsky himself would assume) that understanding questions involves recovering the underlying declarative. This means that the question-transformation must have an *inverse* the child can perform. H_1 is indeed simple, but *its inverse is horribly complicated*. Moreover, *its inverse uses the full resources of the grammar*; all the notions, such as "noun phrase," that H_1 does not employ have to be employed in recovering the declarative from the output of our application of H_1 . So it is no mystery that the child (or its brain) never "tries" such an unworkable semantic theory, and hence never "tries" H_1 .

Incidentally, H_1 itself employs "abstract" notions, since it contains the phrase-structure concept "declarative," and applying it, if it were a rule of English, would therefore involve working with notions such as "noun phrase," since these have to be used to recognize declaratives. And some languages do have question-transformations that are as "structure-independent" as H_1 is; for example, in Hebrew one can form a question

from a declarative by just prefixing *na im*. But this prefixing operation *does* have a simple inverse, namely, deleting *na im*.

I would like now to discuss Chomsky's more abstract remarks at the beginning of his paper. Let me begin with what he says about intelligence.

Chomsky on General Intelligence

So far I have assumed that there is such a thing as general intelligence; that is, that whatever else our innate cognitive repertoire may include, it *must* include *multipurpose* learning strategies, heuristics, and so forth. But Chomsky appears to deny this assumption explicitly. I quote:

More generally, for any species *O* and cognitive domain *D* that have been tentatively identified and delimited, we may, correspondingly, investigate *LT(O,D)*, the "learning theory" for the organism *O* in the domain *D*, a property of the genetically determined initial state. Suppose, for example, that we are investigating the ability of humans to recognize and identify human faces. Assuming "face-recognition" to constitute a legitimate cognitive domain *F*, we may try to specify *LT(H,F)*, the genetically determined principles that give rise to a steady state (apparently some time after language is neurally fixed, and perhaps represented in homologous regions of the right hemisphere, as some recent work suggests). Similarly, other cognitive domains can be studied in humans and other organisms. We would hardly expect to find interesting properties common to *LT(O,D)* for arbitrary *O,D*; that is, we would hardly expect to discover that there exists something that might be called "general learning theory." As far as I know, the prospects for such a theory are no brighter than for a "growth theory," intermediate in level between cellular biology and the study of particular organs, and concerned with the principles that govern the growth of arbitrary organs for arbitrary organisms.

The key notion in this argument is the notion of a "domain." How wide is a domain? Is all of mathematics one domain? If so, what about empirical science? Or are physics, chemistry, and so on, all *different* domains?

If Chomsky admits that a domain can be as wide as empirical science (that there can be a "learning theory for empirical science"), then he has granted that something exists that may fittingly be called "general intelligence." (Chomsky might retort that only exceptionally intelligent individuals can discover new truths in empirical science, whereas everyone learns his native language. But this is an extraordinarily elitist argument: the abilities of exceptionally intelligent men must be *continuous* with those of ordinary men, after all, and the relevant mechanisms must be present at some level of functioning in all human brains.) Even if only physics, or just all of solid-state physics, or just all of the solid-state physics of crystals is one domain, the same point holds: heuristics and strategies capable of enabling us to learn new facts in these areas must be extraordinarily multipurpose (and we have presently no idea what they are). Once it is granted that such multipurpose learning strategies exist, the claim that they *cannot* account for language learning becomes highly dubious, as I argued long ago.⁶ (Consider Washoe!)

On the other hand, if domains become so small that each domain can use only learning strategies that are highly specific in purpose (such as "recognizing faces," "learning a grammar"), then it becomes really a miracle that evolution endowed us with all these skills, most of which (for example, higher mathematics, nuclear physics)

were not used at all until *after* the evolution of the race was complete (some 100,000-odd years ago). And the analogy with organ growth does not then hold at all: the reason there does not have to be a multipurpose learning mechanism is that there are only limited numbers of organs, whereas there are virtually unlimited numbers of "domains."

The Prospects of General Learning Theory

Chomsky feels that the "prospects" of "general learning theory" are bad. I tend to agree. I see no reason to think that the detailed functioning of the human mind will ever be transparent to the human mind.⁷ But the existence of general intelligence is one question; the prospect for a revealing *description* of it is another.

Incidentally, if the innateness hypothesis is right, I am also not optimistic about the prospects for a revealing description of the innate template of language. The examples Chomsky has given us of how to go about inferring the structure of the template (such as the argument about H_1 and H_2) are such bad arguments that they cast serious doubt on the feasibility of the whole program, at least at this point in history (especially if there exist *both* general intelligence *and* an innate template).

On the other hand, we may well be able to discover interesting facts and laws about general intelligence without being able to describe it completely, or to model it by, say, a computer program. There may be progress in studying general intelligence without its being the case that we ever succeed in writing down a "general learning theory" in the sense of a mathematical model of multipurpose learning.

Chomsky on Evolution

Chomsky dismisses Piaget's question regarding how such a thing as an innate template for language might have evolved. But he should not dismiss it. One answer he might have given is this: primitive language first appeared as an *invention*, introduced by some extraordinary member of the species and learned by the others as Washoe learns her fragment of language. Given such a beginning of the instrument, genetic changes to enable us to use the instrument better (including the enlargement of the so-called speech center in the left lobe of normal humans) could have occurred, and would be explained, if they did occur, by natural selection. Presumably Chomsky did not give this answer because (1) he wants to deny that there exists such a thing as general intelligence, and to deny that even the simplest grammar could be internalized by general intelligence alone; and (2) he wants to deny that Washoe's performance is continuous with language learning, and to deny that it has any interest for the study of language learning. But this is surely perverse. If the first language user *already* had a complete innate template, then this could only have been a miraculous break in the evolutionary sequence, as Piaget in effect points out.

Chomsky remarks that we don't know the details of the development of the motor organs either, and this is surely true. We do postulate that they developed bit by bit. This poses difficulties, however, since there are no creatures with two thirds of a wing! But there have been impressive successes in this direction (for example, working out the evolution of the eye). We have found creatures with gliding membranes which are, in a sense, "two thirds of a wing." And we have found eyes with only rods (no cones) and eyes with only cones (no rods). Since the first draft of this paper was written, there have been exciting new suggestions in evolutionary theory.⁸

It is one thing to say that we cannot scientifically explain how certain structures were produced (and the theory of natural selection does not even claim that those structures were *probable*), and quite another to say that we now have scientific reason to postulate a large number of "mental organs" as specific as the various domains and subdomains of human knowledge. Such a mental organization would not be scientifically explicable at all; it would mean that God simply decided to produce these structures at a certain point in time because they were the ones we would need a half a million (or whatever) years later. (Although I don't doubt that God is ultimately responsible for what we are, it is bad scientific methodology to invoke Him as a *deus ex machina*. And, in any case, this is such a *messy* miracle to attribute to Him! Why should He pack our heads with a billion different "mental organs," rather than just making us smart?) On the other hand, if our language capacity did develop bit by bit, even with "jumps," a description of the first bit will almost certainly sound like a description of Washoe. But then we will have conceded that *some* internalization of linguistic rules (at least in prototype form) can be accounted for without innateness.

A Better Argument

But this suggests that there *is* an argument for *some* "innateness" that Chomsky might have used. Consider the phenomenon called "echo-location" in the bat. The bat emits supersonic "noises," which are reflected from the prey (or whatever—for example, a single insect), and the bat can "steer" by these sound-reflections as well as if it had sight (that is, it can avoid fine wires, catch the mosquito that is trying to avoid it, and so forth). Now, examination of the bat's brain shows that there has been a tremendous enlargement of the centers connected with hearing (they fill about seven-eighths of the bat's brain), as compared to other mammals (including, presumably, those in its evolutionary past). Clearly, a lot of the bat's echo-locating ability is *now* "innate."

Suppose Chomsky were to grant that Washoe has protospeech, and thereby grant that general intelligence can account for *some* language learning. He could then *use evolution as an argument for (some) "innateness."* In other words, we could argue that, given the enormous value of the language ability (as central to human life as echo-location is to bat life), it is *likely* that genetic changes have occurred to make the instrument better—for example, the development of the "speech center" in the left lobe. (But caution is needed: if the left lobe is damaged early, speech *can* develop in the right lobe.) This argument is the only one I know of that makes it plausible that there is *some* innate structuring of human language that is not simply a corollary to the innate (that is, genetically predetermined) structuring of human cognition in general. But the argument is not very strong: it could be *general intelligence* that has been genetically refined bit by bit and not a hypothetical language template. Indeed, even species-specific and functionally useless aspects of all human languages could be the product of unknown but genetically predetermined aspects of the overall functioning of the human brain and not clues to the character of a language template; so the mere existence of such aspects is no evidence at all for the template hypothesis.

I think there is an answer that Chomsky can make to this objection; but I will defer it until I have discussed Piaget.

Piaget's "Constructivism"

The view I have been putting forward—that everything Chomsky ascribes to an innate template of language, a "mental organ" specifically designed to enable us to *talk*, can,

for all we know, be explained by general intelligence—agrees in broad outline with the view of Piaget. However, there seem to me to be serious conceptual difficulties with this view when it is combined with Piaget's specific account of what general intelligence is like.

Piaget supposes that human intelligence develops in stages, each stage depending on biological maturation (that is, the age of the child) and on the successful attainment of the previous stages. At a certain stage, certain concepts characteristically appear, for example, the concept of "conservation." But what is it to have such a concept as conservation?

I submit that the only coherent account presently available for having the concept of conservation is this: to have the concept is to have mastered a bit of *theory*, that is, to have acquired the characteristic uses of such expressions as "same amount," and some key *beliefs*, expressed by sentences involving such expressions, or equivalent symbolism. I don't claim that all concepts are abilities to use symbolism; an animal that *expects* the water to reach the same height when it is poured from a pot back into the glass might be said to have a minimal concept of conservation, but I claim that anything like the *full* concept of conservation involves the ability to use symbolism with the complexity of language in certain ways. (I don't claim that this is a "tautology"; rather that it is the only coherent account presently available for what full-blown concepts *are*. And I don't claim to have argued this here, but I have discussed this elsewhere;⁹ and, of course, this insight is not mine but Wittgenstein's—indeed, it is the main burden of *Philosophical Investigations*.)

But if a maturational schedule *involving the development of concepts* is innate, and *concepts are essentially connected with language*,¹⁰ then Piaget's hypothesis would seem to imply Chomsky's; "constructivism" would entail "nativism."

Of course, Piaget does not commit so crude an error. He does not suppose that the maturational *schedule* is given (that is, innate); what he takes as given is "reflective abstraction"—it is this that "precedes language" and that is supposed to take us from one "step" to the next.

But "reflection" and "abstraction" have *no literal meaning* apart from *language*! If "reflective abstraction" is not literally meant, it is either a metaphor for empiricist "generalization," which is insufficient to account for language learning and use, or a metaphor for we-know-not-what.

It seems to me that Piaget should take the view that "reflective abstraction" is something *like* the use of language in the making of hypothetico-deductive inferences, as Chomsky and Fodor urge, and hence conclude that something *like* the use of language is "innate." This position would have brought him into convergence with Chomsky, instead of into an unnecessary sectarian squabble. Moreover, his own suggestion in 1958 that formal logic is the best model for human reasoning¹¹ is very consonant with such a position.

Fodor's "Tautology"

In the discussion Fodor said some things that were a little careless. I want to rectify some of these errors, not for the sake of being "picky," but because the discussion becomes hopelessly confused at the critical point if we let them stand.

First a quibble: Fodor and Chomsky are simply wrong when they say that it is a "tautology" that we can't learn anything, unless some innate "prejudices" are "built in". It is not *logically* impossible that our heads should be as empty as the Tin Woodman's and we should still talk, love, and so on; it would just be an extreme example of a

causal anomaly if it ever happened that a creature with no internal structure did these things. I don't doubt for one moment that our dispositions *do* have a causal explanation, and of course the functional organization of our brains is where one might look for a causal explanation (although I myself think that we won't be able to describe this in very much detail in the foreseeable future).¹² But this still is not a tautology.

Second, it is true that we can't learn how to learn unless we have some prior learning-dispositions: we have to have some dispositions to learn that are not themselves learned, on pain of infinite regress (however, the impossibility of an infinite regress in the real world is hardly a tautology!); but that does not mean that it is *logically necessary* (a "tautology") that the unlearned dispositions be innate. We might (*logically possibly*) acquire a new *unlearned* disposition every five minutes for no cause at all, for example, or for some cause that does not count as a form of "learning." There just aren't any significant tautologies in this area.

The reason this is not *just* a quibble is this: once we pare down Fodor's and Chomsky's big "tautology" to something like this: *as a matter of fact* (not logic!), *no learning without some laws of learning*, we see that no one, least of all the empiricists, has ever denied it. Chomsky's and Fodor's claim that there is a big, mysterious tautology that no one appreciated until Nelson Goodman and that everyone they dislike fails to appreciate is mere rhetoric.

Fodor's Argument for the Innateness of All Concepts

My aim in the remainder of this paper is to develop a modest a priori argument for the Fodor-Chomsky view that something like a language-processing capacity must be innate. But if Fodor's argument in *The Language of Thought* were acceptable, my work would be all done. So I must first explain why I reject Fodor's argument.

Fodor's argument has two parts. First, he contends that the only model we have presently available for the brain is the all-purpose digital computer. He contends, moreover, that such a computer, if it "learns" at all, must have an innate "program" for making generalizations in its built-in computer language. (Here he goes too fast—this is precisely what I think we need an *argument* for.) Second, he concludes that every predicate that a brain could learn to use must have a *translation* into the computer language of that brain. So no "new" concepts can be acquired: all concepts are innate!

I want to examine this second part of the argument, which is fallacious even if the first part is granted. Fodor's reasoning is as follows: Learning the meaning of a predicate is inferring what the semantic properties of that predicate are, that is, concluding (inductively) to some such generalization as:

(A) For every x , P is true of x if and only if $Q(x)$.

But if (A) is in brain language, so is Q . (P need not be; P is mentioned, not used in (A). But Q is *used*, not mentioned.) And if (A) is correct, Q is coextensive with P , and is so by virtue of what P *means* (otherwise (A) is not a correct semantic characterization of the meaning of P). So Q is *synonymous* with P ; P is not a *new* concept, because there is a predicate (namely, Q) in "brain language" that is synonymous with it. But P is an *arbitrary* predicate the brain can learn to understand—so no new concepts can be learned!

What is wrong with this argument is clear. The assumption is as strong as what Fodor wishes to prove. So all we have to do is show how it could be false, even given Fodor's general outlook, and nothing is left but a simple case of begging the question.

First a point of terminology: Every computer does have a built-in "computer language," but *not* a language that contains quantifiers (that is, the words "all" and "some," or synonyms thereof). Let me explain.

A digital computer is a device that stores its own program and that consults its own program in the course of a computation. It is not at all necessary that the brain be a digital computer in *this* sense. The brain does not, after all, have to be reprogrammed as an all-purpose digital computer does. (One might reply that learning is "reprogramming"; but Fodor is talking about the program *for* learning, not about what is learned, and this program might be stored as the brain's *structure*, not as a code.) Waiving this objection: the program that a digital computer stores consists of "instructions" such as "add the two numbers in address 12" and "go back to step 6"—none of which use the word "all." So generalization (A) *cannot ever be stated* in "machine language," even if the computer's program is a program for making inductive inferences in some formalized language (for example, if the program is that of the hypothetico-deductive machine mentioned earlier). Moreover, machine language does not contain (nor can one introduce into it by definition) such notions as "tree," "cow," "jumps," "spontaneous," "pert," and so on—it only contains such notions as "add," "subtract," "0," "1," "put result in address 17," "go back to instruction so-and-so," and "print out contents of address blah-blah."

Let us suppose, however (what needs to be proved) that our brain is a hypothetico-deductive machine, and that it carries out inference in a formalized language ILL (for Inductive Logic Language) according to some program for eliminative induction. And let us suppose that Fodor is not really talking about the brain's *machine language* when he postulates his "language of thought," but about ILL. Even if so strong an assumption is conceded, his argument still does not work.

To see why it does not work, let us recall that when the speaker has finally mastered the predicate *P*, on Fodor's model, he is supposed to have acquired a new "subroutine." Even if this subroutine is described initially in ILL or in some special "programming language," or both, it has to have a translation into machine language that the brain's "compiler" can work out, or the brain won't "execute" this subroutine. Let *S* be the description of the subroutine in question in *machine language*; then even if we grant that the brain learns *P* by making an induction, it need not be an induction with the conclusion (A). It would suffice that the brain instead conclude:

(B) I will be doing OK with *P* if subroutine *S* is employed.

And *this* can be stated in ILL provided ILL has the concept "doing OK with an item," and ILL contains machine language. But this does *not* require ILL to contain (synonyms for) "face," "cow," "jumps," "spontaneous," "pert," and so on. Fodor's argument has failed.

Fodor suggests that he would claim that the machine language description of how to use, say, "tree" is (a form of) the predicate *tree*. But this is simply an extension of use designed to make his thesis an uninteresting "tautology."

Of course, the predicate "doing OK with *P*" may arouse suspicion. But it should not. The "machine" (the brain) doesn't have to understand this predicate as linguists and philosophers would! The generalization (B) is simply a signal to the machine to add subroutine *S* to its repertoire of subroutines. (We should keep in mind Dennett's caution that talk of "machine language" is dangerous because we are tempted to confuse *our* abilities with the formalism in question with the machine's abilities.)

Notes

1. Z. S. Harris, *Methods in Structural Linguistics* (Chicago: University of Chicago Press, 1951).
2. N. Chomsky, "Explanatory Models in Linguistics," in *Logic, Methodology and Philosophy of Science*, ed. E. Nagel, P. Suppes, and A. Tarski (Stanford, Calif.: Stanford University Press, 1962).
3. Each formula can be associated with a corresponding statement expressed in ordinary language, namely, "not-A," "A and B," "A or B," "if A, then B."
4. By "declarative grammar" I mean that part of the grammar that generates the declarative sentences of the language. The usual assumption—made also by Chomsky—is that interrogatives, imperatives, and so on are somehow derived from declaratives.
5. What I have given here is a very oversimplified account of Washoe's actual abilities. The interested reader should consult the following works: B. Gardner and R. A. Gardner, "Two-Way Communication with an Infant Chimpanzee," in *Behavior of Non-Human Primates*, ed. A. Schrier and F. Stollnitz (New York and London: Academic Press, 1971), vol. 4, pp. 117–184; B. Gardner and R. A. Gardner, "Teaching Sign Language to the Chimpanzee Washoe" (16-mm sound film), State College of Pennsylvania, Psychological Cinema Register, 1974; B. Gardner and R. A. Gardner, "Comparing the Early Utterances of Child and Chimpanzee," in *Minnesota Symposia on Child Psychology*, ed. A. Pick (Minneapolis, Minn.: University of Minnesota Press, 1974); B. Gardner and R. A. Gardner, "Evidence for Sentence Constituents in the Early Utterances of Child and Chimpanzee," *Journal of Experimental Psychology: General* 104: 244–267, 1975. The last of these references bears directly on Washoe's ability to learn "structure-dependent" rules.
6. See chapter 5 of my *Mind, Language, and Reality* (Cambridge: Cambridge University Press, 1975).
7. I discuss this in my 1976 John Locke Lectures, *Meaning and the Moral Sciences* (London: Routledge and Kegan Paul, 1978).
8. For an account of some of these suggestions, I recommend Stephen Gould's *Ontogeny and Phylogeny* (Cambridge, Mass.: Harvard University Press, 1977).
9. See chapter 1 of my *Mind, Language, and Reality*.
10. It is worth noting in this connection that Piaget's research method (in all but a few experiments) consists of studying *verbal* behavior.
11. J. Piaget and B. Inhelder, *The Growth of Logical Thinking from Childhood to Adolescence* (London: Routledge and Kegan Paul, 1958).
12. This is argued in my John Locke Lectures.